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THE HISTORY OF PHYSICS*

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THE beginnings of anything like a connected history of the science which is now called physics may be placed with considerable definiteness about the beginning of the 17th century and associated with the great name of Galileo. It is of course true that innumerable isolated facts had been known for many centuries which are now included among the data of this science; and many tools and simple machines which are now regarded as applications of physical principles had been devised and used. Even prehistoric man knew some of these—to his very great advantage. But, with one important exception which will be mentioned later, there was, in the ancient world, no connected body of knowledge in this field which can properly be called scientific. In this respect physics differs radically from mathematics, or astronomy, natural history, or medicine, each of which began its modern career with a store of scientific knowledge that had been obtained and put in order before the Renaissance.

The reason for this difference is doubtless to be found in the fact that the progress of physics is dependent, almost from the first step, on the method of experiment as distinguished from the method of observation. For some unknown psychological reason, the appreciation of the possibilities of experiment as an intellectual tool and the ability to make use of its technique appear very late in the history of human development. A few individuals like Archimedes understood and practiced it, and it is difficult to understand why the seed which they sowed proved sterile. Certain inhibitions, common (despite their very different temperaments) to the Greeks, the Romans and the men of the Middle Ages, seem to have prevented the infection from spreading from its original foci. I have no theory to offer as to the cause of the removal of these inhibitions during the 16th and 17th centuries; but whatever the cause we must, I think, recognize that

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about that time a new factor made its appearance in the intellectual world which has survived and grown and has produced momentous results.

Some of you no doubt will be disposed to question the novelty which I have attributed to the methods used by Galileo and his successors. You will say with truth that men have been experimenting since long before the dawn of history; that by this means they improved their weapons, food, clothing, shelter and means of transport so that the enormous advantage in material surroundings which the Roman of the Augustan Age had over the prehistoric cave dweller may properly be said to be the result of a long course of progressive experimentation. But what I have, for the sake of making a distinction, called the experimental method in science is a very different thing from the slow empirical improvement of tools and appliances which went on before the beginnings of the modern era. The two kinds of activity differ fundamentally in the objects which they seek to attain and in the means they adopt for the accomplishment of their purposes.

The difference of objective is well illustrated by a remark of Galileo's at the very beginning of one of his most important works, the "Dialogue Concerning Two New Sciences." He says that he has long thought that the workmen in a great shop such as the Arsenal at Venice must know many things which would be of great service to philosophers if they could only be persuaded to use them. And he does in fact take such workman's knowledge got by the old empirical process and employ it for a purpose for which it had never been used before; to find out for example something about the laws and regularities governing the strength of materials and their dependence upon the size and shape of the object considered. The knowledge thus gained might or might not be useful to the workman, but it was of great consequence to the philosopher. Every scientific experiment has an objective of this kind—that is, every one that can properly be called an *experiment*, that has in it an element of originality and adventure into the unknown and that is not a mere routine test by known methods. Its purpose is much more general than the improvement of a tool or a telephone; and because of its generality it may incidentally do more to improve implements and technique in widely diverse fields of industry than thousands of experiments of the old cut-and-try kind extending over many centuries. The geometrical rate of increase resulting from this is sufficiently obvious from the industrial history of the past hundred years. Its continuance however is strictly conditioned upon the retention of the attitude of Galileo's philosopher; he may glance out of the corner of his eye at the by-products of his work but he must not think too much of them and must keep clearly in view his own philosophical mission.

As I have said the modern experimental method differs from the older empiricism in procedure as well as in purpose. The actual experiment is only a part of the process and does not come first in order of time; before it can be begun advantageously, there must be much careful thought and planning which often involves mathematics and deductive reasoning of the most old-fashioned kind. But there are no artificial hazards and rules of the game, such as those which the Greeks were so fond of imposing in mathematical problems. Any sort of logic (or the lack of it) is permissible since the final test is to be the experiment and not consistency of argument; it will indeed be a test of the premises no less than of the reasoning process. The greatest masters are those who make most use of apparently non-logical processes—intuitions and "hunches" which are perhaps the results of subconscious reasoning from data but dimly perceived.

The experiment itself is an observation made under highly artificial and carefully prearranged conditions, and it is this which gives the method its greatest advantage over simple observation of natural phenomena. This is well illustrated by Galileo's work upon the principles of mechanics and in their application to the particular case of the motion of falling bodies. Centuries of inescapable observation of moving bodies had led to no correct idea of the simple laws underlying their behavior, because these laws had been obscured by the effect of friction—a secondary condition of the problem. Galileo's experiments consisted in reducing these effects until the true nature of the phenomena could be observed. The famous experiment at the Leaning Tower of Pisa was a spectacular demonstration of one point of his theory designed to confute his Aristotelian critics; but the really important and fertile experments were quite simply arranged with the help of iron balls, inclined tracks, boards, nails and bits of string. With the simplest material means he laid the foundations of dynamics and, with it, those of physical science as a whole. Lagrange remarks that Galileo's contributions to mechanics "did not bring him in his lifetime as much celebrity as those discoveries which he made about the system of the world, but they are to-day the most enduring and real part of the glory of this great man. The discoveries of Jupiter's satellites, of the phases of Venus, of sun spots, etc., needed only telescopes and assiduity; but extraordinary genius was needed to disentangle the laws of nature from phenomena which are always going on under our eyes, but of which the explanation had always eluded the search of philosophers."[1]

[1]The necessary brevity of this lecture may result in giving the impression that Galileo had no forerunners. This is of course not the case. Archimedes has already been mentioned—a lonely genius who laid firmly the foundations of the statics of solids and liquids. Stevinus, sixteen years older

The world was ready for the structure which was to be erected upon the foundations laid by Galileo In the next generation, Torricelli in Italy and Pascal in France showed by bold reasoning and experimentation that Nature's *horror vacui* was due to the weight of the atmosphere; while Guericke in Germany and Boyle in England discovered other important properties of gases. In dynamics, the direct succession fell to Christian Huygens of Amsterdam, a natural philosopher of very high rank and a worthy successor of Galileo. He completed the theory of the pendulum and by its use determined the acceleration of gravity; invented and constructed the pendulum clock and escapement, discovered the theorems of centrifugal force, and was the first to use what is now called the principle of *vis viva* or kinetic energy. His investigations in optics are also of great importance and he was one of the first proponents of the wave theory of light.

To try to give in a small fraction of a single lecture any adequate account of the mighty deeds of Newton is, of course, to attempt the impossible. Fortunately the main features of his achievements are so familiarly known that a brief recapitulation is all that is necessary. Born in 1642, the year of Galileo's death, his genius developed with extraordinary rapidity. It appears to be quite certain that the essential parts of all his great discoveries were made before he was twenty-five years old, although most of them were published much later. The delay was due partly to lack of facilities for publication but mainly to Newton's carefulness in verification and in working out all possible consequences of his hypotheses. His first great discovery (made in the year in which he received the bachelor's degree at Cambridge) was the "direct and inverse methods of fluxions" which are in all essentials identical with the differential and integral calculus, but with a less convenient and fertile notation. This discovery belongs of course primarily to the history of mathematics; but both physics and astronomy may proudly claim it as belonging partly to them for two reasons; first, because it was the exigencies of their problems which led directly to it; and second, because it was an absolutely indispensable tool for the mechanical and astronomical discoveries of Newton and his successors. In the same year (1665) Newton "began to think of gravity extending to the orb of the moon"; he soon found from one of Kepler's laws that the forces which keep the planets in their orbits must vary inversely as the square of their distances from the sun. He applied this rule to the earth and moon and found an approximate agreement between the force necessary to keep the moon

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than Galileo, made notable additions to the Archimedean statics. And a century earlier Leonardo da Vinci (a miracle of versatility) had made important discoveries in statics, and had found the true law of the refraction of light. Most of Leonardo's scientific work, however, remained buried in his manuscript notes and has only recently been revealed to the world.

in her orbit and the force of gravity at the surface of the earth. "All this," says Newton in later life, "was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy more than at any time since."

During the twenty-one years which elapsed before the publication of the *Principia* in 1687, Newton, in the midst of other duties and of investigations on other subjects, recurred again and again to the astronomical and dynamical problems which had engaged his youthful attention. It was only after ten or twelve years that he cleared up the difficulties of centrifugal force (Huygens' previous work being then unknown to him) and thus discovered that the two remaining laws of Kepler were consequences of his gravitational law. In the last three or four years of the period under consideration he appears to have worked steadily at the development of the subject and to have discovered the large number of important theorems and relations which make the *Principia* the most stupendous and overwhelming publication in the history of science.

In all this work there are three streams of discovery which may be separated by logical analysis, but which are so closely intermingled that it is difficult to see how any one of them could have gone on without the other two. Nobody has ever been able to believe that Newton could have extended the Galilean dynamics to the intricate motions of the planets except by the aid of the method of fluxions or its equivalent; and certainly nothing could have been done without a knowledge of the law of gravitation. On the other hand, the solution of astronomical problems required and facilitated a more exact formulation of the principles of mechanics than Galileo had been able to give; although mathematically intricate, they are dynamically simpler than terrestrial problems since no appreciable frictional or dissipative forces are present; and they furnish tests and verifications of dynamical laws of far greater accuracy than can be obtained in any other way.

Under such conditions it seems futile to attempt to decide whether physics is most indebted to Newton for the formulation of the laws of motion, for the discovery of the law of inverse squares, or for the invention of the fluxional calculus. Any one of the three (if it could have been produced alone) would have made his name immortal; the fact that we owe all three to one person places him upon a pinnacle of greatness which has not even been approached by any other man of science.

I must not neglect to mention also Newton's contributions to optics which, while not of the fundamental importance of those we have just been discussing, were nevertheless worthy of their author. I need only to recall to your memory that he investigated the composition of white light, the colors of thin films, diffraction, and the possibilities of

achromatism in refracting telescopes. He was not infallible; for he decided that it was impossible to make an achromatic refractor, and he supported the corpuscular theory of light against the undulatory theory of Huygens. In both cases, however, the evidence obtainable in his time strongly supported his position; and I think it was this, rather than the mere authority of his name, which caused the corpuscular theory to prevail during the following century.

In the eighteenth century the development of mechanics and of gravitational theory was carried on by the three Bernoullis, Euler, Clairaut, d'Alembert and others. This development reached its culmination near the end of the century in the publication of Lagrange's "Mécanique Analytique" and of the "Mécanique Céleste" of Laplace—works which for completeness and finish have seldom if ever been excelled. The period was not characterized by new discoveries of the first magnitude but by the careful working out of the theories founded by Galileo and Newton and the perfection of mathematical methods for dealing with complicated problems. This was an admirable preparation for the great outburst of discovery which began toward the end of the eighteenth century and was continued still more conspicuously in the first years of the nineteenth.

In other branches of physics and in chemistry, the ground was also being prepared in a different way by the accumulation of experimental facts and relations which formed the raw material for the generalizations of the period which was to come, and served as starting points for notable advances. It is necessary therefore to go back and to trace briefly the course of the tributary streams of discovery which were soon to join the main current. We shall have to consider what had been learned about magnetism, electricity, light and heat.

The ancients were acquainted with the curious property possessed by the lode-stone of Magnesia of attracting iron, and also knew that when amber was rubbed it attracted bits of straw and other light bodies. Nothing came of this knowledge for centuries; but at some unknown time prior to the Crusades, the north-seeking property of the magnetized needle was discovered and the mariner's compass was invented. The science of magnetism was indeed almost the only part of physics that made any progress during the Middle Ages. In the thirteenth century Petrus Peregrinus of Picardy, experimenting with a spherical lode stone and a needle, found that the stone possessed two "poles" which appeared to be the seat of the magnetic power.

The true founder of both magnetic and electric science, however, was William Gilbert of Colchester, physician to Queen Elizabeth. Twenty-four years older than Galileo, Gilbert must be regarded as one of the pioneers of the experimental method. His work had not the scope and depth which characterized that of the great Italian, nor were

its consequences so immediate and so revolutionary; but he was nevertheless a truly scientific experimenter and, considering the time in which he lived, we must regard him as a prodigy of originality. He showed quite conclusively that the behavior of the compass was due to the fact that the earth itself was a great magnet. For two thousand years it had been supposed that amber alone was capable of being excited by friction to attract other bodies; Gilbert found that many bodies could be thus excited and that among them were such commonplace substances as glass, sulphur and resin. His experiments and his reasoning were sound, and he devised a hypothesis of "electric effluvia" which was helpful to electrical science for a long time.

During the eighteenth century the experimental knowledge of both electricity and magnetism progressed rapidly. The conduction of the electrified state by metals was discovered by Stephen Gray; the Leyden jar was invented; du Fay found that there existed two opposite states of electrification which he called vitreous and resinous and that these behaved, as to attractions and repulsions, like the two poles of a magnet. America made her first contribution to physics in the very important work of Benjamin Franklin. Toward the close of this period of activity the doctrines of electric effluvia and of Cartesian vortices had been definitely replaced by the theory that the forces observed were due to action at a distance between charges of a single electric fluid and matter (Franklin) or to a similar action between two fluids (Coulomb). Eventually the law of variation of this force with the distance was experimentally determined by Coulomb and found to be the familiar inverse square relation of Newton. The same law was shown by Coulomb to hold for the forces between magnetic poles also; and either one, or two, magnetic fluids had to be predicated to account for the variation in the strength of magnets. Indeed the application of gravitational theory to these forces rendered inevitable the introduction of such imponderable fluids to take the place of the material masses which play the same rôle in the case of gravitation.

I have already mentioned briefly Newton's researches in optics and his adherence to the corpuscular theory in which he was followed by most philosophers. This introduced another "imponderable" which was however supposed to consist of excessively minute discrete corpuscles instead of a continuous fluid. Many important optical phenomena had been discovered. Descartes had published the mathematical law of refraction which however was not his own discovery but was apparently communicated to him by Snell of Leyden; Newton had discovered and properly interpreted the composite nature of white light and had investigated the simpler cases of diffraction and of what is now called interference; Huygens had observed double refraction in Iceland spar and had given the proximate explanation of

it (on the wave theory) which is still current; and he observed that the two beams which had passed through the spar differed from each other and from ordinary light in the peculiar way which we now indicate by calling them polarized. Time is lacking for any discussion of the ingenious arguments by which the two rival theories of light were supported.

The quantitative study of heat begins with Galileo's construction of the first thermometer—an air-thermometer of considerable sensitiveness but of inconvenient design. Improvements of one kind or another were made by many men and the fixity of certain temperatures (such as that of melting ice) was established. The first really reliable thermometers were made by Fahrenheit in the first quarter of the eighteenth century. All of the early thermal experiments and theories were confused by the failure to distinguish clearly between temperature and quantity of heat, and by the great and apparently capricious differences between the heat capacities, or specific heats, of different substances. All these difficulties were finally cleared up in a masterly manner by Joseph Black near the end of the period we are considering. He established calorimetric measurements on a firm basis, and showed quite clearly that in all such experiments heat behaves like a substance which passes from one body to another and sometimes becomes "latent" for a time (as when ice melts) but which is never created or destroyed. All these conclusions are true within the range of Black's experiments; and it was only at a later date that the exceptions were seen to be of great importance and not explicable by appealing to latency or to a variation in heat capacity. What was known at that time thoroughly justified a substantial theory of heat as the most convenient hypothesis available; and thus another imponderable fluid took its place as a respectable and useful article in the physicist's creed. Many unavailing attempts were made to show the identity of two or more of these hypothetical substances. Thus, on account of the phenomena of radiant heat, it was proposed to identify caloric with the light corpuscles; but the fact that light passed through glass while radiant heat did not, was an insuperable obstacle to this view. There was always in the background the possibility that both heat and light were forms of motion, but at the period now under consideration the substantial theories undoubtedly held the field. This state of things led to very sharp boundaries between the different fields of physics and discouraged the natural inclination to apply the principles of dynamics (which by this time had come to seem almost intuitive) to other physical phenomena. There was no great encouragement to apply the principles of mechanics to the imponderables; so far as experiment showed they lacked not only the conspicuous property of weight but also the most essential dynamical characteristic of

ordinary matter, inertia. The natural and fertile method of dealing with them was to take as postulates for the mathematical development of the subject certain empirical relations as simple and fundamental as possible. It was not until the establishment of the conservation of energy in the late forties of the nineteenth century that the barriers between the different "physical forces" were broken down.

The intervening period however was one of brilliant discovery in both mathematical and experimental physics. In the theory of heat two works of this period must at least be mentioned. In 1822 Joseph Fourier publisher his *Théorie Analytique de la Chaleur* a work of genius which has had a profound effect in almost all branches of theoretical physics and upon pure mathematics as well. A still more momentous event in the history of science was the publication in 1824 of Carnot's *Réflections sur le Puissance Motrice du Feu.* His primary purpose was the investigation of the efficiency of heat engines which had recently become a matter of interest owing to the increasing use of the steam engine invented by Newcomen and Watt. In this paper Carnot makes use of an analogy; he saw that the production of work by an engine might be regarded as due to the fall of caloric from a higher to a lower temperature just as the work of a water mill is a consequence of the fall of water from a higher to a lower level. He follows a course of reasoning so simple yet so effective that it seems inspired; it is based upon the denial of the possibility of perpetual motion which even at that time was a pretty firmly established empirical fact owing to the consistent failure of all attempts to produce such motion. He thus establishes the general principle which is now called "the second law of thermodynamics" and which is of far wider application than could have been imagined by Carnot or any of his contemporaries. For it happens that nearly every phenomenon in the physical universe is attended by an evolution or absorption of heat and is therefore subject to the second law. It governs every chemical reaction as was shown by Willard Gibbs fifty years later, and the physical and chemical processes of life. It sets bounds to cosmological speculations and to forecasts of the future of the human race. Not the slightest deviation from it has ever been observed and the probability of such deviation is so minute that it must be regarded as one of the most firmly established of scientific facts.

In electrostatics and magnetism this period was marked by the development of the mathematical consequences of Coulomb's discovery that the inverse square law applies to these forces. Much of the gravitational theory could be taken over directly while the special applications to electricity were made by Poisson, Green, and others. In the meanwhile however another set of electrical phenomena had appeared. In 1791 Galvani, professor of anatomy at Bologna, gave an

account of his experiments on the contraction of frogs' legs when touched with two different metals in series and, with much ability, supported the view that it was an electrical manifestation. He naturally supposed that the origin of the electrical disturbance was in the animal tissues. This was combatted a year later by Volta who referred the seat of the forces involved to the point of contact between the dissimilar metals, and gave good evidence that they were electrical. The effects however were very small, and interest flagged until 1800 when Volta invented the "pile" by means of which very appreciable results could be obtained. This at once excited much attention; and in the same year Nicholson and Carlisle in England in experimenting with the Voltaic pile observed the decomposition of water by electrolysis and shortly afterward Humphrey Davy advanced the chemical theory of the pile, which, after many years of struggle, eventually superseded the contact theory of Volta. There was a rapid advance in the knowledge of the electric current, of batteries, and of the electrolytic process. These experiments produced a profound effect upon chemistry through the electrochemical theory of Berzelius; and although this has long been given up, the most modern theories have, in a different form, reverted to the view that chemical forces are of electrical origin.

Many attempts had been made to discover some connection between the phenomena of electricity and those of magnetism but all had failed until 1820 when Oersted of Copenhagen observed and correctly described the action of an electric current upon a magnet brought near it. As soon as the news of this observation reached Paris, Ampère began the series of investigations which was to render his name immortal in electrical science. Within a week he had demonstrated to the Academy the attractions and repulsions of parallel currents; and during the ensuing three years his brilliant experimental and mathematical researches laid a sure and firm foundation for all the subsequent developments in electrodynamics. As was to be expected, he based his investigations on the Newtonian model, by using current-elements acting upon each other by forces in the line joining them. Again the law proved to be that of the inverse square; but the fact that the attracting elements were directed quantities added many difficulties which, in the state of mathematical science at that time, gave ample scope to the "Newton of electricity" for the display of his genius. The vector relations involved in the statement of his problem caused an indeterminateness which later gave rise to many rivals to Ampère's expression for the force between current elements. These all gave the same result when integrated around closed circuits which alone were amenable to experiment; and no one could succeed in devising experiments which would discriminate between them. One of these rival theories, that of Weber, is interesting as being in some respects similar to the modern theory of electrons.

Great as is the debt which electrical science owes to Ampère, it is exceeded by its obligation to Faraday whose marvellous experimental skill and instinctive perception of the inner nature of phenomena are still the wonder and admiration of all men of science. At twenty-one years of age he was a journeyman book-binder who had educated himself in some degree by reading the books which he was given to bind. The Encyclopaedia Britannica aroused his interest in science and he applied to Davy for employment in the Royal Institution. For a number of years, as Davy's assistant, his chief work was in chemistry; but Oersted's discovery turned his thoughts toward electricity and thereafter it was his principal field of work. In 1831 he made the capital discovery of the induction of currents which is not only of the most fundamental consequence to the theory of electromagnetism but is the foundation of the innumerable practical applications of electricity to the uses of man. Of his many other discoveries I shall mention only two; the quantitative laws of electrolysis which bear his name and which gave the first suggestions of an atomic theory of electricity, and the specific inductive capacity of dielectrics.

Because of the deficiencies of his early education, Faraday never acquired the technique of the mathematician. But, as Maxwell has pointed out, his mind was admirably fitted for dealing with quantitative relations. He overcame the handicap under which he suffered by devising his own methods of representing the quantitative side of phenomena—methods which not only enabled him to achieve his unparalleled success as a discoverer but which are so useful to others that they have held the field in elementary instruction in electromagnetism as well as in the most complicated problems of modern electrical engineering. His lines of force were to him real entities and he conceived of all forces as being transmitted from point to point in a continuous medium. The idea of action at a distance was repugnant to him. It is indeed to most physicists but Faraday was not tempted as most of us are to use distance forces because of their mathematical convenience and thus to escape the prodigious difficulties of imagining a medium with the necessary properties to account for the forces. Faraday's prejudices were to have important consequences in the next generation as we shall see when we come to speak of Maxwell.

The year 1800 is an important date in the history of optics as it is in that of electricity; for in that year Thomas Young took up the cudgels for the wave theory of light which had been almost completely neglected since the time of Huygens. In the following year he explained the colors of thin films (Newton's rings) by means of the "interference" of waves; and in 1803 he applied the same idea to certain problems of diffraction, but in a way which was afterward proved to be wrong. He was drawn into a controversy with the great Laplace

who had worked out a theory of double refraction on the corpuscular basis; and for a dozen years or more Young found little sympathy and support for his views among scientific men of established reputation. Indeed he was far from having a good case; the explanation of diffraction was not satisfactory; there was no explanation of polarization since waves in the tenuous and fluid ether were quite naturally supposed to be compressional like sound waves in air; and, for the same reason, no satisfactory explanation of double refraction appeared to be possible.

The first defect was remedied by the work of Fresnel, presented to the Paris Academy in 1816, in which the author began that brilliant series of experimental and mathematical investigations which left the wave theory completely victorous over its rival. He gave the true theory of diffraction by a slit and a wire and showed that it agreed with the results of his experimental measurements. Poisson, who was one of the referees of his paper, noted the fatal objection that Fresnel's theory would require a bright spot in the exact center of the shadow of a circular object. When, however, the matter was put to the test of experiment under suitable conditions, the bright spot was found and this naturally produced a reaction in favor of Fresnel's theory. It appears to have been Young who took the bold step of suggesting that the vibrations in light waves were transverse and that thus polarization could be explained. Fresnel at once took up this suggestion and succeeded in bringing into line all the intricacies of crystalline refraction, including that in biaxial crystals which had been discovered a few years before by Brewster and had been a stumbling block to all other theories. Later he took up the theory of reflection and refraction by ordinary transparent bodies with equal success; and since the completion of his series of memoirs there has never been a doubt in the mind of any competent person that light has the kinematical properties of transverse wave motion.

On the dynamical side, however, matters were not so clear. Only a solid can transmit tranverse elastic waves and it was difficult to believe that the ether could be a solid and yet allow the free motion of material bodies through it without the slightest detectable resistance. This was the origin of the great problem of the existence and properties of the ether—a problem which has excited the most eager interest of physicists for a hundred years and is still with us. Many of the most important discoveries, mathematical and experimental, have arisen from attempts at its solution. It at once stimulated the mathematical study of the theory of elastic solids and of the applicability of this theory to the phenomena of light. The work of Gauchy, Green, McCullagh, Stokes and Kelvin in this field may be said to have created a new era in mathematical physics and even in mathematics itself; for

the treatment of continuous media required methods which differed in many ways from those appropriate to distance forces of the Newtonian type. It was also the first attempt to apply in all strictness the principles of dynamics to natural phenomena outside the restricted field of mechanics proper. It was never perfectly successful, but so nearly that there was constant encouragement to persevere. We shall have occasion to look at a second phase of this gallant attack upon the mysteries of Nature when we come to deal with the work of Clerk Maxwell.

About the middle of the century occurred the epoch-making discovery of the conservation of energy which brought all kinds of physical and chemical phenomena into much more intimate relation with each other than had previously been suspected. Incidentally, it greatly strengthened the tendency, of which I have just spoken, to seek for a strictly dynamical foundation for all such phenomena.

The discovery arose primarily in a reconsideration of the nature of heat, and its history is so curious and interesting that it is with regret that I recognize the impossibility of giving an adequate account of it within the limits of this lecture. As we have seen, the belief that heat was a substantial fluid had prevailed for many years and had proved useful; but there had always been a suspicion (extending back to the time of Hooke and Newton) that it might be an effect of motion—either of the fine particles of which ordinary matter was made up, or of light-corpuscles within matter. At the end of the eighteenth century, Count Rumford had made experiments which ought to have started things in the right direction, but were disregarded. Carnot himself, in some posthumous notes which were not published until 1878, gave so clear an outline of the true theory that we cannot doubt that the course of science would have been greatly altered, as Mach remarks, if Carnot had not died of cholera in 1832. The caloric theory was finally overthrown by the labors of two men, Mayer and Joule, quite independently and neither having in the beginning any knowledge of the work of the other. Mayer, a Jewish physician of Heilbronn, began his process of reasoning with the observation that venous blood is a brighter red in tropical than in temperate climates. He was so ignorant of the terminology of physics that he could not make himself understood at first and suffered many rebuffs in consequence. His persistence however was sublime; he learned to write so that physicists could understand him, unearthed forgotten experiments, and eventually, without any experiments of his own, gave conclusive evidence for his theory and obtained a good value of the mechanical equivalent of heat. There could scarcely be a greater contrast than that between him and his fellow discoverer. Joule was a Manchester brewer and amateur of science, a skilful and accurate experimenter who

year after year turned out unimpeachable quantitative evidence of the equivalence between mechanical work and heat in all sorts of transformations. A third collaborator in placing the new theory on a firm foundation was Helmholtz whose celebrated memoir of 1847 showed clearly the generality of the new principle and its applicability to all branches of science; he gave it suitable mathematical formulation and demonstrated its great power in finding relations between phenomena of apparently different kinds.

The next step was the reconciliation of the new principle with that of Carnot, and it proved to be a difficult one. It puzzled Kelvin for several years and delayed his complete adherence to Joule's theory; ultimately he saw his way clearly and as a result of his work and that of Clausius the modern theory was established upon the two principles which stand side by side as the first and second laws of thermodynamics. These two empirical principles are probably the most firmly established and most thoroughly verified of all the so-called laws of nature. In the classical treatment of the subject they are regarded as axioms, and deductions are made from them so that, in form, the science is like geometry. As I have previously intimated, the results obtained are of great generality and of far-reaching consequence in practical applications as well as in philosophical implications. It is one of the great triumphs of theoretical physics.

Side by side with this theory there grew up another method of dealing with the subject which was less general and more hypothetical but has proved to be an invaluable aid to research. As soon as it was recognized that heat and mechanical energy are mutually convertible, it became inevitable that physicists should seek for a detailed mechanical theory of heat. The obvious hypothesis was that heat consisted of the energy of motion of the small particles, or molecules, of matter whose existence had been more or less generally accepted since Dalton's introduction of the atomic theory to account for the chemical laws of definite and multiple proportion. In order to develop this theory, the laws of mechanics had to be applied statistically to enormous aggregates of molecules reacting upon each other in various ways. The simplest state of matter from this point of view is the gaseous one; and in the hands of Clausius and Maxwell the kinetic theory of gases made great progress in a few years. Atomic and molecular theory became at once definite and quantitative. One of Dalton's atoms might be of any size so long as it was small enough to escape individual observation and had the correct ratio of mass to other atoms; but the atoms and molecules of the physical theory had definite and calculable mass, size, velocity and free-path. They became very real to physicists and were constantly used in reasoning and in planning experiments.

About twenty-five years ago a determined attack upon all atomic theories was made by Ostwald and his followers among the physical chemists—largely through ignorance of the real evidence upon which they were based. They ridiculed such theories as metaphysical figments of the imagination and attacked them as obstacles to real advance in the philosophy of nature. The faith of physicists however was not for a moment shaken; and it has been justified by the progress of discovery in the intervening years. The last doubting Thomas has been convinced and only those who deny the objectivity of matter itself can now question the real, physical existence of atoms and molecules.

Through the labors of Boltzmann, Gibbs and others, the application of statistical mechanics to molecular problems was developed and generalized so as to be applicable to other states of matter than the gaseous one; and attempts were made to reduce the whole of thermodynamics to a mechanical basis. The subject is a very difficult one with many pitfalls for even the most wary; and we must conclude, I think, that the attempt has met with a defeat that is probably final. It has, however, led directly to the quantum theory of Planck, a great generalization which is the most puzzling and the most promising treasure in the possession of the physicist of today.

The next great landmark of which we must take note is the unification of the theories of electrodynamics and of optics by Clerk Maxwell. He himself tells us that, impressed by the value and fertility of Faraday's ideas, he decided, in beginning his serious study of electricity, to read no mathematics on the subject until he had mastered Faraday's "Experimental Researches." Maxwell was a highly trained and original mathematician and his first papers on electrodynamics were devoted to the expression in clear mathematical form of some of Faraday's hypotheses and modes of thought. Like his chosen master he rejected action at a distance and concentrated his attention upon the hypothetical medium by means of which electromagnetic forces might be transmitted. In several memoirs published during the sixties he gave details of mechanical models which were adapted to this end. By gradual steps these auxiliaries were done away with and at the same time the theory far outgrew its original purpose of translating Faraday into mathematical language. Maxwell showed clearly that all the known facts of electrodynamics could be attributed to the action of a medium and by strict mathematical reasoning he deducted the properties which this medium must have. These turned out to be identical in all details with those which we must attribute to the luminiferous ether in order to account for the phenomena of light. Thus was born the electromagnetic theory of light and two great domains of physics were brought together under a single system of hypotheses clearly expressed in the form of differential equations.

The publication of Maxwell's "Treatise on Electricity and Magnetism" in 1873 was an event of the first importance in the history of science. The new theory was slow in making its way, especially on the continent of Europe, and Maxwell himself died in 1879. His work was taken up, however, by a group of devoted adherents among who we may mention Heaviside, Lodge, Rowland, Poynting, Gibbs, J. J. Thomson, and Larmor. In 1886 Hertz, whose attention had been some years before directed to Maxwell's theory by Helmholtz, made an accidental observation which to his acute mind offered the possibility of a direct test of the finite speed of propagation of electromagnetic action. His brilliant series of experiments demonstrated the existence, speed, and properties of electromagnetic waves and served as a complete verification of Maxwell's theory. All of you know that the wonders of wireless are a direct consequence of the experiments of Hertz; but to the physicist this is less interesting and significant than the steady growth in scope and authority of Maxwell's equations, which come nearer to the ideal of a "world formula" than anything else known to the modern man of science.

For something like ten years it was generally supposed that the main outlines of the science of physics had been drawn in fairly satisfactory, and perhaps final, form. There was still much to be done but it would be concerned with details—with perfecting theories and increasing the accuracy of measurements. A great deal of very valuable work of this kind was done in many fields; as an example I may refer briefly to the development of accurate measurement in spectroscopy.

The use of the spectroscope as a method of chemical analysis was placed on a sound basis about 1860, by Bunsen and Kirchhoff, and the application of this method was extended, by the brilliant discovery of Kirchhoff, to the atmospheres of the sun and stars. You all know something of the wonderful results which have followed the application of the spectroscope to astronomical problems and of the growth of the borderland science which is called astrophysics. Great improvements in spectroscopic apparatus were made by Rowland, Michelson, and others, and there grew up a body of skilful spectroscopists, who devoted themselves to the accurate measurement of the wave lengths of the innumerable spectral lines given out by the different chemical elements and to the discovery of empirical relations between the numerical values of these wave lengths. It was hoped that such observations would throw light upon the structure of atoms but for many years no progress was made in this direction. Indeed it is only recently that the results of a generation of spectroscopists are beginning to be useful for this purpose and only after the clue to a theory of atomic structure had been given by investigations in other fields. Spectroscopy is almost the only part of physics in which a

large mass of data was accumulated before the existence of a guiding hypothesis or theory to direct the work. The method of simple induction and classification which has played so large a part in some other sciences seems to be unsuited to the problems of physics.

Accurate measurements, however, do sometimes produce brilliant discoveries—when they fall into the right hands. A classical example of this is the discovery of argon by Lord Rayleigh as the result of a quite prosaic undertaking to redetermine with great accuracy the density of nitrogen. As a sequel to Rayleigh's work, a whole family of chemical elements, whose existence had been entirely unsuspected by chemists, was discovered by Ramsay. But it is only in rare instances that this sort of thing occurs; usually an accurate measurement leads to no exciting result, but takes its place among the solid foundation stones of the science. And for perhaps a decade there was fairly widespread opinion among physicists that this was what they must look forward to, and that the future of physics lay "in the last place of decimals."

These anticipations of a useful, if somewhat dull, old age for the science were happily disappointed in the last years of the century by the remarkable outburst of unexpected discoveries among which the Röntgen rays came first in point of time. This was followed almost at once by Becquerel's discovery of radioactivity, the identification of the subatomic "corpuscle" or electron by J. J. Thomson, and the investigations of the ionization of gases which have led to many important result. No physicist who has reached middle age can forget the romantic interest of the ten years following 1895, when startling discoveries followed each other in rapid succession and the physical journals were awaited with an impatience not unlike the desire for newspapers in wartime. But the news was all good news, and recorded an almost unbroken series of victories.

These discoveries were, as I have said, unexpected but they were not in any real sense accidental. They came as the result of a careful and prolonged study of the electrical discharge through rarefied gases—a complicated set of phenomena very difficult to put in order. Twenty years earlier, Maxwell had predicted that the next great step in our knowledge of the relations between electricity and matter would come from a study of the discharge through gases; and it had been prosecuted in that spirit by many men though the clue which they sought eluded them for twenty years. When it did come at last, it was in a form which was, so far as I know, entirely unpredicted and unexpected. This was so much the case that it took us more than fifteen years to find out quite certainly just what the X-rays were. It was not until 1912 that Laue's discovery of the diffraction of X-rays by crystals and the subsequent work of W. H. and W. L. Bragg made it quite certain that these

rays were of the same nature as light but with wave lengths only about 1/5000 of those in the visible spectrum. This had indeed been for some time the prevailing hypothesis as to their nature but there was little quantitative evidence to support it; and only a year or two previous to the discovery of crystalline diffraction W. H. Bragg himself had brought forward many reasons for thinking that X-rays might be corpuscular. The study of these very short waves has already given us invaluable knowledge of the nature of the atoms of different elements and promises still greater advances in the future; it has provided a new and powerful method of studying crystal structure and has revolutionized our conception of the nature of chemical combination in crystalline bodies; and it promises to have practical applications as useful in industry as ordinary spectroscopy.

The discovery of the radioactivity by Becquerel followed almost immediately upon Röntgen's discovery of the X-rays, and was in a sense a direct consequence of it; they are alike too in that they have both had important medical applications which have drawn much public attention. Madame Curie's sensational discovery of radium was an early incident in the history of this subject. But by far the most important development in this field was the establishment by Rutherford and his pupils of the cause and source of energy of these radiations. He has shown in the most conclusive way that they are due to the disintegration of the atoms of the radioactive elements—uranium, thorium, radium, etc—and that a spontaneous transmutation of these elements is going on constantly. The genealogy of the radioactive elements is known more accurately than that of most royal families; and the birth and mortality statistics of the various kinds of atoms are in all the text books. Thus a part of the dream of the alchemists has come true, but only a part; for up to the present all attempts to produce artificially the transmutation of the heavy elements have failed. In fact we have not been able to affect in the slightest way the spontaneous transmutation of the radioactive elements; it can neither be retarded nor accelerated by any agency at our command. We do know, however, that vast stores of energy are locked up in the atoms of the heavier elements and if the time should ever come when this can be released and controlled by man it will doubtless cause a revolution in industrial processes more fundamental than that which followed upon the introduction of steam and electricity. One small step in this direction has been taken within the past two years. Rutherford has obtained evidence that the nitrogen atom may be broken up by bombardment with alpha rays, and that one of the products of this process is hydrogen. It is perhaps too early to regard this as being definitely established; and, even if it be true, the amount of matter transmuted in this way is excessively minute while the quantity of energy released in the process (if any) is far be-

low what could possibly be measured experimentally. We have however become accustomed to small beginnings which ultimately produce great results; and a modern physicist would be rash indeed who should attempt to set bounds to the possibilities of future discovery in this direction.

The discovery of the electron was also an event of the first importance in the history of our science. It is the ultimate atom of negative electricity and is a constituent of all material atoms. It can also exist in the free or "disembodied" state, as for example in the cathode rays, the beta rays from radium, and in the electronic stream from incandescent bodies. In the last of these forms it has proved to be of great practical use to telephony and wireless telegraphy in the audion or thermionic tube which is the cause of most of the remarkable advances in these fields during the past five or six years. To the physicst and chemist of today the electron is an indispensable concept in both theoretical and experimental investigations; and its reality can be questioned only on those philosophical grounds which may put in doubt the existence of matter itself.

The nature of positive electricity is not so definitely known; but evidence is accumulating that it too exists in an atomic form as the "nucleus" of the atom of hydrogen—the residue left when the hydrogen atom is deprived of its single negative electron. It is becoming probable that the "nuclei" of other atoms are built up out of these and of negative electrons. If this group of hypotheses should stand the test of time we shall have to conclude that matter and electricity are different aspects of the same stuff—that the atoms of matter are formed by different collocations of the atoms of positive and negative electricity.

Another line of physical inquiry which has proved to be of deep and fundamental significance is the so-called quantum theory of Planck. It originated in the study (both experimental and theoretical) of the intensity and quality of the radiation from a "black body," or perfect radiator, when held at a definite temperature. The total intensity of such radiations were deduced theoretically by Stefan from the principles of thermodynamics and the predicted results have been amply verified by experiment. When however the attempt is made to predict the way in which the energy is distributed in the spectrum, so as to be able to tell what fraction of the total intensity is carried by any particular wave length, the problem becomes much more difficult. It is necessary to have recourse to statistical methods analogous to those used by Maxwell, Boltzmann and Gibbs in accounting for the thermodynamic properties of material bodies. First steps in this direction were taken by W. Wien but the deductions from his theory were not altogether in accord with experimental results. Planck succeeded in ob-

taining a formula which agreed with experiment, but only by making certain very daring hypotheses; the most conspicuous of these is that the emission, or the absorption of radiation, or both, takes place not steadily and continuously as we had always supposed but by finite, discrete "quanta." From one point of view this hypothesis of Planck may be regarded as extending the field of the atomic theory, hitherto restricted to matter, to energy as well. I can not hope to suggest even remotely in the brief time at my disposal how revolutionary Planck's assumptions really are; they are still very imperfectly understood and it has not yet been possible to reconcile them wholly with other facts and general laws which appear to rest upon very solid foundations. Indeed, if the results of Planck's speculations had been confined to the deduction of a formula for the radiation of a black body they would not, I think, have long engaged the serious attention of physicists. But they began to turn up unmistakably in many other fields of investigation—for example, in connection with the photoelectric effect, with X-rays, and in all theories of atomic structure. At present no one doubts that most of our fundamental ideas in mechanics and electrodynamics must be revised in the light of the quantum theory which however is itself still in a very immature state. The problem thus arising of bringing together under one system apparently discrepant bodies of phenomena is an exceedingly difficult one and we may have to wait for another Newton to solve it. But it possesses the greatest fascination for all theoretical physicists; they are able to congratulate themselves upon the possession of an unsolved problem of the first magnitude and of great difficulty and they know that as long as it lasts, life will not be dull for them.

I should be in despair if it were necessary to give, at the end of a lecture already too long, an account of Einstein, relativity and gravitation. Fortunately any need that you may feel for instruction on these subjects has doubtless been satisfied by the newspapers, the magazines, and by innumerable books, popular and otherwise. Let me say in all seriousness however, that the more one knows of the history and recent developments of physics the more sincere and ardent is one's admiration for the individuality and brilliant originality of Einstein's genius. It does not seem probable at present that his discoveries will have as great an effect upon the immediate future of physics as some of the others which I have just discussed. But the ultimate result of his work upon *methods* used in the theoretical side of physical science may well prove to be revolutionary; and it seems highly probable that it will change to some extent our philosophical views of the nature of the external world and of our relation to it.

It may have occurred to you that, in my hurried sketch of the progress of physics since 1895, I have made very frequent use of such

terms as "important," "epoch-making," or "revolutionary." The truth is that all the various discoveries and theories which arose more or less independently and have been separately mentioned are constituent parts of one "revolution" which has not yet reached its climax. It is one of the greatest intellectual pleasures of the present day physicist to see how all these apparently diverse things are fitting into each other and taking their appropriate places in a general scheme which is rapidly assuming form and coherence. The new ideas in physics are having a profound influence upon the fundamental theories of chemistry and are bringing the philosophies of the two sciences much closer together. They have already made possible a rational theory of the periodic law of Mendelejeff, and have displaced the atomic weight as the controlling factor in the determination of the chemical properties of the elements. They have also given grounds for a very reasonable hope that the near future may see the development of a real theory of chemical combination, which is certainly much to be desired.

The general character of the profound change which is taking place in the fundamental ideas of both sciences may perhaps be stated briefly and inadequately in the following terms. The recent discoveries in physics have enabled us to experiment in several ways with the individual atom and to find out something of its properties and activities. Until recently we have been able to deal only with statistical averages of the behavior of vast numbers of atoms and molecules and all of our physical laws have been based upon such statistical knowledge. The apparent discrepancies between the older and the newer formulations may well be due to this difference. It is quite possible that the ultimate laws which govern the actions of atoms are quite different from the laws of mechanics and electrodynamics which are so familiar that they seem almost axiomatic. If this should be so, the familiar "laws" will in no way lose their validity within the field that they have ruled so long; but we shall know that they are not fundamental and primary, but secondary statistical laws in which much of the individuality of physical activities has been ironed out by the process of averaging. To come to this point of view is of course rather a wrench for those of us who have been nursed and reared in the old régime. But this discomfort is much more than compensated for by the fascinating and apparently inexhaustible field for research and speculation which is now being opened up for our use and pleasure.